

In this the arrows indicate the direction of an imagined plane or intra-molecular interface i separating the hydrophile groups $\begin{array}{c} \text{C}-\text{N} \\ \cdot \quad \cdot \\ \text{O} \quad \text{H} \end{array}$, which are con-

solute with water (in virtue of residual affinities tending to complete the amino- and carboxyl groups), from the hydrophobe or hydrocarbon groups —CHR. Not only in one and the same protein molecule, but also to a variable extent between molecules, we may admit that this primary orientation leads to mutual attraction between water-soluble and water-insoluble groups respectively. Without any actual cleavage of the molecule, we have orientation and a straticchemical field of force which is of a similar character, in essence, to crystallisation, but results in incomplete instead of complete equilibrium. The hydrocarbon or lipid atom groups will approach the fluid on the solid state according to molecular weights and constitution; hence the system may be likened, in one aspect, to a sub-molecular emulsion, the lipid groups tending to form interconnected sheets of atom-groups necessarily permeable to water and water solutes, although mechanically developing a stress resisting rupture in virtue of the fields of attraction and repulsion induced. The micelles are the smallest plurimolecular units thus built up.

Applied to soaps, we have similarly a mutual attraction and solution of the hydrocarbon portions of the fatty acid radicles, without cleavage from the water-soluble portion, which dissolves and ionises. The passage of ionised micelles "through the open network of the gel as freely as through the sol" (Laing and McBain, *loc. cit.*, p. 1519) appears quite consistent with the hypothesis now suggested. Further, the form of the micellar aggregates—strings, sheets, networks of molecules—will, on this view, be a function of the original molecular constitution operating through intra-molecular orientation, and modified by ionisation and tautomerism, where these occur. The quasi-solubility in water of sodium, etc., soaps, being associated with ionisation, passes to insolubility in water with the non-ionising calcium, aluminium, iron, etc., soaps, when the solubility of the fatty acid portion (or hydrocarbon group) becomes dominant, and soap sols in non-aqueous solvents result. The stiffening to gels here with increased concentration and lowered temperature may be due to orientation of both the hydrocarbon and the metallic residues respectively, of the latter either directly or as oxides, these being solids at such temperatures.

In general, it is submitted that the present hypothesis gives a more generalised basis of explanation of what McBain regards as "not yet explained" (*loc. cit.*, p. 1518), viz. "the stable existence of any colloidal aggregate."

S. E. SHEPPARD.

Research Laboratory, Eastman Kodak Co.,
Rochester, N.Y., January 18.

THROUGH the kindness of the Editor I have been given an opportunity of commenting upon the speculations advanced by Dr. S. E. Sheppard in the foregoing interesting letter.

It is evident from reference to his paper in the *Journal of Industrial and Engineering Chemistry* that Dr. Sheppard is tempted to diverge from the views of nearly all who have studied the properties of suspensoid colloids, such as fine suspensions of particles of gold in water, and to regard these as being merely pseudo-colloids. In his opinion, a jelly made from gelatin, protein, starch, or soap would be the typical colloid, thus reverting to Graham's conception that

it is the substance, and not the physical state of subdivision, that makes a colloid.

It is impossible to exclude ionisation hypotheses from colloid chemistry now that it has been demonstrated that soaps in colloidal form are excellent conductors. At the same time we are quite clear that a theory of gels cannot depend upon ionisation phenomena, since gels occur in non-aqueous solvents which possess no measurable conductivity.

It is difficult to understand exactly what is meant by some of the technical terms used or coined without definition; but apparently Dr. Sheppard's conception of a stable colloid is a substance which contains atoms or atomic groupings, commonly found in chemicals which are insoluble in the solvent under discussion. For example, in aqueous sodium palmitate the long paraffin chain is regarded as being in itself insoluble in water, in contradistinction to the sodium atoms and ions. This is considered to result in a tendency for these hydrocarbon chains throughout the solution to become linked to each other through the residual affinity of the paraffin part of the molecule, to form sheets of molecular network co-extensive with the solution. The sodium end of each molecule is regarded as "dissolved" and subject to ionisation.

This conception is sufficiently elastic to conform to many of the facts, but surely such a word as "dissolved" loses its significance when applied to a solution in which both "undissolved" and "dissolved" parts of the molecule are present in a state of molecular subdivision. Thus, in the case of an aqueous solution in which gold is present in the form of single atoms—a case which has been very nearly realised—the gold would not be regarded as dissolved in the water, since gold and water are "heterochemical." The modern or current conception is certainly that this would be a true solution of gold, although highly supersaturated. It is evident that the difference is one of words, and not of scientific fact.

Again, it would be difficult to explain on Dr. Sheppard's conception the existence of gels such as that of rubber in benzene, in which surely every part of the hydrocarbon must be considered potentially soluble or "consolute" with benzene. Further, on what chemical grounds could one predict the formation of a gel of cadmium in alcohol?

I cannot but feel that even this conception of continuous open molecular network as constituting the typical colloid still leaves unexplained the stable existence of the colloidal aggregates of sols as distinguished from gels. In the case of an ordinary soap solution or sol, for example, perfect reversible equilibrium prevails, and yet the soap does not exist as "a continuous, semi-rigid framework," nor yet as single, independent molecules—that is, as crystalloid—since when in the latter condition it exhibits familiar crystalloidal properties such as osmotic activity. Hence our conclusion is that the soap is largely in the form of particles, each an aggregation of large numbers of molecules. Miss Laing found that there is a very ready change from sol to gel without alteration of either conductivity or osmotic activity. We seem forced, therefore, to conclude that the gel is built up from the same colloidal particles as the sol.

Similarly, in Svedberg's example of cadmium or cadmium oxide in alcohol, which at rest forms a jelly but on stirring reverts to a fluid sol, the colloidal particles of the sol must undoubtedly be those of the gel also. In this case the individual colloidal particles are presumably crystalline, in analogy with the experimental results recently obtained in Sherrer's X-ray investigations.

On the other hand, such colloids as gelatin have not indicated any regular pattern when examined by X-rays. A fully developed network of oriented molecules such as Dr. Sheppard describes should give indications analogous to a crystalline structure when thus examined. This X-ray method of investigation is being applied in another department of the University of Bristol to the various forms of soap solutions. It is hoped also to obtain fresh light on the problem by the experiments now being carried out by Miss Laing on the conduction of continuous current through soap jellies.

Dr. Sheppard's demand that any "consistent" theory of colloids should permit of the deduction of all the physical properties from the chemical formula alone, appears to over-estimate the extent to which the manifold physical properties of gold and silver sols of different degrees of subdivision and colour can be deduced merely from the knowledge of the chemical formulæ of the metals. In conclusion, I think his idea is at present too vague and not sufficiently in accord with such facts as those mentioned to be likely to prove more fruitful than the one it seeks to replace, incomplete as the latter is in the absence of further experiment.

JAMES W. MCBAIN.

The Chemical Department, University of
Bristol, February 24.

The Production of Living *Clavellina* Zooids in Winter by Experiment.

IN a recent publication ("Sea-temperature, Breeding, and Distribution in Marine Animals," Journ. Mar. Biol. Assoc., vol. xii., No. 2, p. 351) the present writer showed that there was every reason to believe that the hibernation phenomena in many marine animals are purely temperature effects. In order to test this view the positions of sixteen good colonies of the beautiful Ascidian *Clavellina lepadiformis* were marked on September 1, 1920, on the wooden piles of the West Wharf, Great Western Docks, Millbay, Plymouth. This Ascidian usually appears on these piles about the end of May and dies down about the end of October, and has never been recorded in winter. On September 15 and 30 the piles were again visited and a record was made of those colonies which had survived the marking. The positions of the colonies were found to be shown effectively by three long wire nails driven into the piles on the outside of the colonies at the apices of imaginary triangles. On February 23 last the laboratory collector, Mr. Wm. Searle, who assisted in the marking of the colonies, visited the piles at the West Wharf and took careful scrapings between the nails marking the positions where *Clavellina* colonies were seen in September, 1920.

The material obtained remained in the collecting honey-jars on the floor of the laboratory until 8 p.m. of February 24. It was then examined, and anything like a resting stage of an Ascidian was picked out, cleaned a little, and transferred to clean water in a glass dish. On February 25 at noon the material was put into a warm room at a temperature of about 61° F., and distributed in a number of finger-bowls in ordinary tank-water passed through a Berkefeldt filter.

Little attention was given to the bowls beyond changing the water on February 28, until March 1, when a distinct *Clavellina* zooid was found in one dish and a bud in another. From that date onwards the number of zooids and buds has increased, and at the latest observation made on March 8 there were

twelve living zooids or well-developed buds and two well-developed zooids had been preserved. From the beginning of the experiment to March 1 the temperature did not fall below 60° F., and from an inspection of the thermograph records the mean temperature of the room is seen to be very nearly 61° F.; probably the mean temperature of the water in the dishes would be slightly lower. Since March 1 the mean temperature of the room and water has been slightly higher.

It is therefore highly probable that the awakening of *Clavellina* from the resting stage is a pure temperature effect. In this experiment tank-water was deliberately used, and it is considered highly improbable that this water can be regarded as biologically better than the water now surrounding the sleeping stages of *Clavellina* in the sea. There remains, therefore, only the presence or absence of some recondite chemical complex in the water as a possible factor in aiding in the awakening of this Ascidian. The existence of such a complex is, however, not regarded as probable.

Driesch has shown that *Clavellina* regenerates lost parts with facility, and that starving or foul water will also cause this Ascidian to absorb all its organs and pass into an undifferentiated condition. It would appear, however, that none of these factors operate during the period of hibernation, since the water at the West Wharf is undoubtedly more foul during the period when *Clavellina* flourishes than when it passes into and remains in the resting condition, and similar Ascidians in the same locality feed and grow during the winter. Other forms which feed in the same way, and probably on the same kinds of food, as *Clavellina* also flourish and grow in the same situation in winter.

It would therefore seem that variations in temperature are the normal stimuli for development and differentiation in *Clavellina*, and the determination of the actual point in temperature at which these changes occur should afford a useful clue in attacking the question of the underlying chemico-physical changes.

The winter resting stages of *Clavellina* are very simple bodies; they are flattened expansions of transparent gelatinous material (tunicin) with a mammillated surface containing a core of opaque yellow tissue—apparently undifferentiated—which shows mammillations corresponding to those in the gelatinous coat. In the development of the zooids the mammillations swell and a core of tissue extends into the swelling. The bud thus formed increases in size and differentiates into the zooid.

J. H. ORTON.

The Laboratory, The Hoe, Plymouth,
March 9.

The Elementary Particle of Positive Electricity.

REGARDING the suggestions for the name of the hydrogen nucleus made by Prof. Soddy (NATURE, December 16, 1920, p. 502) and Dr. Prideaux (NATURE, December 30, 1920, p. 567), it would seem to be better to use the term "hydron" instead of "hydron," as being shorter and more euphonious.

It may be recalled that the late Lord Kelvin used himself, and tried in vain to induce others to use, the term "electrion" instead of "electron." At this late date it seems quite unnecessary to insist on the retention of the extra syllable simply to have the word "ion" retained in the longer term unless for the sake of euphony, as in "thermion."

ANDREW H. PATTERSON.

University of North Carolina, February 19.